Thomas Kuhn, Modern Mathematics and the Dynamics of Reason

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Abstract

It is now 30 years since a group of philosophers and historians published a collection of articles (Gillies (ed.) 1992) which took as their central question the usefulness for the philosophy of mathematics of Kuhn's constructions in the philosophy of science. Important work was done to see whether the concepts of 'revolution' and 'paradigm' made sense there. However, a feature of the collection which should raise doubts about the universality of its findings was the restriction, except in a single case, to work done prior to 1900. In this article I discuss what form a historically-oriented philosophy of *modern* mathematics should take, and the role it could play in developing Michael Friedman's post-Kuhnian theory as described in his 'Dynamics of Reason' (2001) and later publications.

Keywords— Mathematics, Thomas Kuhn, Michael Friedman, post-Kuhnianism

1 Introduction

We live in exciting times. The ever-evolving demands of mathematics, acting over a great many years, have prompted the formulation of a new foundational outlook. Instead of taking sets as the basic shapes underlying all others, collections of distinct and yet indistinguishable points, the new theory – *homotopical algebra* or *higher category theory*[1] – describes shapes with a richer structure, known as *homotopy types*. Not only that, we now also have a logical formalism that can directly treat these homotopy types, naturally known as *homotopy type theory* (UFP 2013), a name which does double duty by bearing witness to the fact that it is a variety of type theory.[2] We now need philosophers with a good grounding in dependent type theory and category theory to interpret this language and its modal variants for their colleagues to see what this new logic might allow them to achieve. My own contribution (Corfield 2020) to a small number of studies leaves a very great many avenues unexplored. Prospects for powerful applications in metaphysics and the philosophy of language are bright.

We would have been better prepared for this eventuality had it been expected of those philosophers tasked with understanding issues arising from mathematics that they attend to currents within active mathematical research, especially in the core branches of geometry, algebra and analysis. Where historically-informed philosophy of science has gone from strength to strength over many decades, in no small part due to Kuhn's seminal texts, it has been much harder to persuade Anglophone philosophers to care about the history of *mathematics* as a guide to the proper treatment of the subject. This is all the more the case where it comes to what I'm calling here *modern mathematics*, which I'll take for the purposes of this article to be that dating from 1930. Nothing much hangs on this particular date, but once we have established the philosophical interest of modern mainstream mathematical theories, we should be in a position to push back against any thought that there's little to see philosophically in mathematics beyond set theory, that given by, say, the axioms of ZFC. If philosophy needs to know about Alexandre Grothendieck's transformation of algebraic geometry through the 1960s, it has very little to do with the adequacy of ZFC to represent it and much to do with his innovative *functorial* outlook.[3]

One way to establish that modern mainstream mathematical theories *are* philosophically interesting is to produce telling case studies about them along the lines of historically-informed philosophy of science, by applying, say, a Kuhnian perspective to modern mathematics. To frame my discussion in this article of the possibility of such an approach, it will be useful in the remainder of this section to give an account of my own quest for such a practice.

My introduction to the philosophy of mathematics came from what to the Anglo-

[1] "The title of this book is less about putting Higher Category Theory and Homotopy Theory side by side, than observing that Higher Category Theory and Homotopical Algebra are essentially the same thing." (Cisinski 2019, viii-ix)

[2] I shall be using the name 'homotopy type theory' broadly to indicate any of a number of related variants of the 'Book HoTT' of (UFP 2013), such as cubical or simplicial HoTT, or Higher Observational Type Theory.

[3] His *functor of points* approach to schemes provides the basis for many developments in modern geometry.

phone community is an unlikely source. I was living in Paris at the time when it was suggested to me that I take a look at the work of Albert Lautman (1908-44). For weeks in the library of the George Pompidou Centre I poured over their copy of the 1977 edition of his *Essai sur l'unité des mathématiques et divers écrits*. I was transfixed by his enthusiastic appreciation of the appearance of the same 'idea' (capitalized in French by Lautman as *Idée*) across subdisciplines of mathematics.[4]

To give an example of one such study, he sampled instances where the *local* and the *global* treatments of a topic interact, showing how in each case both contribute to the better understanding of a subject by their modes of determination of the other. This allowed him to offer a broad perspective:

> ...we try to establish a connection between the structure of the whole and the properties of the parts by which the organizing influence of the whole to which they belong is manifested in the parts. (Lautman 2011, 102)

For instance, we need both Riemann's global treatment of holomorphic functions via Riemann surfaces along with Weierstrass's local series expansions and analytic continuation in complex analysis. In geometry we need both Felix Klein's global *Erlanger Program* and Riemann's local infinitesimal approach. Both approaches are to be retained, but more importantly they are to be seen as complementary aspects of a single whole. As regards the latter case, Lautman cites the mathematician Elie Cartan,

> Departing from the antagonism between Klein's geometries and Riemann geometry, we arrive after a long detour at this finding that it is in the Riemannian form that Klein's geometries best show their fundamental properties. (Cartan 1927, 222) (Lautman 2011, 106)

Cartan famously weds these two approaches in his own *Cartanian geometry*. Here, as with his cases in general, Lautman had chosen well in that the thematic relations he located continue to make their presence felt in mathematics. For instance, Lautman's local-global examples could be extended today to arithmetic where studies of the local (real and p-adic fields) and of the global (rational field) again interpenetrate and determine each other.[5]

Along with Lautman's writings, I was also intrigued at the time by what I was finding out about category theory. It too concerned similarities across diverse branches of mathematics, but this time expressing these similarities by explicit mathematical constructions, such as *adjunctions*. It seemed to me like a form of logic. Indeed back in 1969 William Lawvere had demonstrated that logic involved a "web of adjunctions", where, for instance, existential quantification occurred as a left adjoint to weakening (Lawvere 1969).[6] Thus logical constructions appear as specific cases of much more general principles. The thesis that logic is a part of mathematics, partaking in common structures, was also espoused by Lautman,

> ...we think that the proper movement of a mathematical theory lays out the schema of connections that support certain abstract ideas that are dominating with respect to mathematics. The problem of connections that

[4]See (Corfield 2010) for a treatment.

[5]For example, the *Hasse principle* covers situations where types of equations have a rational solution if and only if they have a solution in the real numbers and in the p-adic numbers for each prime p.

[6]Given unary predicate $Q(x)$ and binary predicate $P(x, y)$, define a new binary predicate $Q^*(x, y)$ which is logically equivalent to $Q(x)$. Then we may define $\exists$ by specifying that $\exists y P(x, y) \Rightarrow Q(x)$ holds precisely when $P(x, y) \Rightarrow Q^*(x, y)$ holds.

these ideas are likely to support can arise outside of any mathematics, but the effectuation of these connections is immediately mathematical theory. Mathematical logic does not enjoy in this respect any special privilege. (Lautman 2011, 28)

However, whereas Lautman takes these *abstract ideas* to be supra-mathematical, the category theorist takes them to be wholly mathematical.[7]

I hoped then for a fruitful fusion of these interests, which explored the sense one might make of Lautman's examples in category-theoretic terms. Now, how to link this project to then ongoing work of philosophers in the Anglophone world? I knew much was governed here by a Quinean outlook where first-order logic is privileged as a formalism that reveals the entities to whose existence one is committed. This seemed a mistake to me to award such a philosophically special role to what was a facet of mathematics.[8] But I was also aware that some philosophers were looking to the history of mathematics. I was particularly fascinated by the historical considerations of Colin McLarty in his *Uses and abuses of the history of topos theory* (McLarty 1990), which criticises the kind of account that seeks to consider the branch of category theory known as *topos theory* principally as providing some generalised form of set theory, ignoring its roots in algebraic geometry. Where Lautman paid no attention to the socio-historical details of his cases, here in McLarty's work were details of occasions where people met other people to discuss this and that issue. A final piece of inspiration came then in the form of Lakatos's *Proofs and Refutations* (Lakatos 1976). Here mathematical concepts were shaped and improved by lively debate. Again, unlike with Lautman, we followed the course of history in the text, although unlike with McLarty this takes place in the footnotes annotating the fictional classroom discussion.[9]

So there I was, set to carry out some kind of historically-inspired philosophical treatment of developments in modern mathematics, showing how the rise of category theory captured parts of the structure similarity Lautman had described. I should try to replicate the dialectical structuring of concept formation, with a view to closer grounding in the history than Lautman, but dealing with more recent examples than Lakatos. Lacking any training in philosophy, I looked for a place in the UK to study, and so sought somewhere where my interests would fit into existing research. Nobody seemed to be pursuing Lakatos's work in mathematics. The only good choice then for a supervisor was Donald Gillies, working at the time at King's College London, who was in the middle of editing *Revolutions in Mathematics* (Gillies (ed.) 1992), thereby testing out Kuhnian concepts in the context of mathematics. While the book, I noted, contained very little post-1900 work, and none of this non-foundational, the opportunity presented itself to further these researches.[10]

[7]It is natural to wonder had he lived longer, whether Lautman would have embraced category theory, a development unavailable to him, Eilenberg and Mac Lane's *General Theory of Natural Equivalences* appearing in 1945, a year after his death at the hands of the Nazis.

[8]Saunders Mac Lane makes this connection too, expressing skepticism that a left adjoint can play such a fundamental role: "My learned and articulate friend Van Quine has claimed that ontology is served by observing that "to be" is to be existentially quantified. I disagree, and I also doubt if Van realizes that the existential quantifier is a left adjoint–an important observation, again due to Lawvere." (Mac Lane 1992, 120).

[9]I later found that much of the most important part of the history of what became algebraic topology had been omitted.

[10]The latest work appears in the chapter by Herbert Breger (1992). Sec. 3 of my article (Corfield 2011) describes the reappearance of Finsler's ideas in the theory of non-well-founded sets. In mathematics apparently defeated ideas frequently re-emerge.

Other philosophy of mathematics I encountered in London in the early nineties shocked me, in particular Hartry Field's fictionalism, which was then popular. Here I was told that mathematicians were talking of fictional entities when asserting that $2 + 2 = 4$, a statement that therefore turns out to be false. Something, it seemed to me, had gone wrong if this was what philosophy was to make of the richness of the discipline of mathematics. To approach this richness, my starting point for doctoral research was Lakatos, both *Proofs and Refutations* and also his theory of scientific research programmes. The latter, as is well-known, comes from Lakatos's attempt in the philosophy of science to restore some Popperian rationality to Kuhn's much more descriptively accurate account, but one Lakatos took to be irrationalist.

Over the years, I have also looked to apply to mathematics concepts devised by other philosophers of science, such as Ian Hacking, Dudley Shapere and Michael Polanyi, but I have made no direct use of Kuhn's ideas of revolution, crisis, normal science, anomaly, paradigm, incommensurability, etc. On the subject of mathematics in general Kuhn himself had very little to say. My aim here then is to explore the potential relevance to modern mathematics of the work of Kuhn, or of that of some suitable follower.

Having introduced my main theme, the remainder of this article takes place over three sections. Even if I haven't deployed his concepts in my writing, it will be helpful for us to see in section 2 what we might learn from consideration of Kuhn's most iconic concept, the *revolution*. Finding that one needs to select a lengthy time-frame to capture in mathematics the kind of radical revision demanded in pursuing a Kuhnian approach, in section 3, I will then consider some varieties of more localised case study one might look to write. It seems that in mathematics there is more space to research, fewer constraints operating to bring different researchers into direct rivalry. One effect of this is to make our choice of case study more difficult. Where philosophers of science will have iconic moments such as Planck's work on black-body radiation, expecting their readers to have a reasonable background knowledge of these events, there is a much lower expectation that one's readers should know of a specific mathematical episode. In view also of the sheer difficulty in conveying modern mathematics, one feels obliged therefore to give one's case studies some extra force by their possessing some notable feature, such as their exploiting a novel diagrammatic notation or their deploying computers in a new way. In the final section I take up a post-Kuhnian development in the shape of Michael Friedman's *Dynamics of Reason* (Friedman 2001), offering a new exemplar of his schema of revolutionary change outlined there. This involves a large-scale revolution to a great swath of mathematics, allowing the expression of novel physical principles. My claim is that not only can we apply the framework of the *Dynamics of Reason* to mathematics, but by doing so we are in a better place to understand ongoing transformations in the physical sciences themselves, Friedman's starting point.

2 Revolutions in Mathematics

It should come as no surprise that there are periods during which profound shifts in a field, or a subfield, of science occur, shifts occasionally so great that one could say that afterwards the discipline is barely recognisable from before. On the other hand, it would seem to be enormously unlikely that science should display across time some very deep commonality of structure in its moments of profound rupture. Plenty have lined up to cast doubt on such an idea, for instance, the historian of science Peter

Galison writes: "I am uncomfortable with the definite article that begins the title of Kuhn's great work, *The Structure of Scientific Revolutions*" (Galison 1997, 60).

One way that we might expect the structure of profound shifts to change over time is when, as a discipline matures, it accumulates a longer list of successes in its past to accommodate. Then it is highly constrained as it seeks to revise its self-understanding, presented in the narratives it tells of its reasoned progression to its current state and of its future goals.[11] Where a subject such as geology underwent the most radical transformation with the postulation and establishment of what came to be called *deep time*, from where we sit today with our detailed account of the 4.5 billion years of the Earth's existence, it is hard to envisage that the centrality of sedimentary, metamorphic and igneous processes of rock formation will diminish, or that the claim will be questioned that there took place something approaching the Great Oxidation Event as we understand it today.

One way to discover radical change in a mature discipline is by extending the time frame. Where we sometimes can barely recognise the 5-year-old in their earlier form as a toddling 2-year-old, it might require us to add on 20 years to a 40-year-old to see a marked change of appearance and behaviour. With this consideration in mind, and since mathematics had achieved a remarkable degree of maturity by the early 1800s,[12] I consider one of the most important chapters in *Revolutions in Mathematics* to be that contributed by Jeremy Gray on nineteenth-century mathematics:

> ...there was a revolution in mathematics in the nineteenth century because, although the objects of study remained superficially the same, the way they were defined, analysed theoretically, and thought about intuitively was entirely transformed. (Gray 1992, 245)

We are used to revolutions acting rapidly – as with the sudden fall of a longstanding political regime – but we extend the term to historical transformations taking place over long periods, as with the *Industrial Revolution*. Gray's revolution seems to resemble more closely such a change than it does the precipitous fall from power of the Russian or French monarchs in their respective countries' revolutions.

Does this prevent Gray's revolution from being properly seen as *Kuhnian*? It seems to have little enough to do with the resolution of some specific, rapid, anomaly-generated crisis and more to do with the lengthy transformation of practice borne out of years of struggle via the generation of hundreds of pieces of insight. Indeed, Gray writes about his revolution:

> This nineteenth-century revolution resembles the scientific revolution identified by Butterfield in being, long, slow, and without an organized leadership having a specific programme, more than it does Kuhn's examples. As with the scientific revolution, rather than Kuhn's examples, it is more useful to see a loosely connected list of influential writers that a single decisive figure. (Gray 1992, 245)

Note that Gray draws our attention here to speed and intention of the actors as markers of a Kuhnian revolution.

Later in his career Kuhn returned to the subject of revolutions in his article *What are scientific revolutions?* (Kuhn 2000). Here the emphasis is on the *holistic* nature of revolutionary change, "an integrated picture of several aspects of nature has to be

[11]See Corfield (2012) for an argument as to the need for such narratives in sustaining traditions of mathematical enquiry.

[12]Just consider what had been achieved by Gauss in many fields.

changed at the same time" (Kuhn 2000, 29). How scientific language relates to the world also changes, as does the range of entities covered by its terms. And what things in the world are considered to be similar also alters.

Unsurprisingly, we may find mathematical phenomena fitting some of this description. When in 1882 Dedekind and Weber made the association between fields of functions and fields of algebraic numbers,[13] so that their 'ideal theory' could apply to both, they were altering the range of application of terms, along with the kinds of things that are to be considered to be similar – here a function and a number. But this moment is better conceived as part of a longer story, as Gray himself does. However, to do so is to fall foul of a feature retained by Kuhn in the later article, namely, the rapidity of revolutionary change, a continuation of the *Gestalt* switch imagery from *Structure*:

> Though scientific revolutions leave much piecemeal mopping up to do, the central change cannot be experienced one step at a time. Instead, some relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before. (Kuhn 2000, 17)

Gray is casting doubt on both the "relatively sudden" and the "unstructured" aspects of his revolutionary transformation. It lasts a century and is composed of a multitude of interconnected steps. Let's now move forward from Gray's period into the following century.

Writing as a mathematician six decades into the twentieth century, Marshall Stone describes a similar dramatic shift in the whole discipline over the preceding sixty years, one he called a "quiet revolution" (Stone 1961, 715). There is no indication in the article that Stone is alluding to the as-yet unpublished ideas of Kuhn, but his language is similar to Gray's. As with the latter's description of *his* period, we hear from Stone of the ongoing detachment of mathematics from the world in favour of more abstract relations, but what Stone importantly adds to this is a further shift amounting to the discovery of "intimate structural connections". These connections give rise to a profound unity:

> **The unity of mathematics**. The characterization of mathematics as the study of systems comprising certain abstract elements and certain abstract relations prescribed among them shows very clearly the essential unity of mathematics. Nevertheless it cannot adequately suggest **the intimate structural connections which have actually been found among the different branches of mathematics**, as a result of modern researches. During the last fifty or sixty years much has been done to identify and compare the mathematical systems dealt with in algebra, number theory, geometry, and analysis. (Stone 1961, 719-720, my emphasis)

Stone himself was instrumental in this shift. Indeed, consider a very algebraic entity, a Boolean algebra. This is a kind of lattice, so a collection of elements equipped with a meet and a join, satisfying various conditions such as distributive laws. It also has a negation operator. The reader may well have met such an entity when considering the collection of propositions of some propositional theory, ordered by implication. Now there's a construction that collects the so-called *prime ideals* of the Boolean algebra and places a topology upon them. This produces a certain kind of topological space, now known as a *Stone* space. So intimate is the connection that

[13] See Corfield (2003, Chap. 4).

in fact all Stone spaces are captured by this construction. And there is an inverse process where given a Stone space, its clopen subsets (both closed and open in the topology) form a Boolean algebra. This result is perfectly captured by category theory in what is now called *Stone duality* – the duality between the category of Boolean algebras and the category of Stone spaces.[14] Such intimate connections have since been found across the whole of mathematics. This marks a great change from the mathematics of the year 1900. Criticising a reductive set-theoretic understanding of mathematics, another mathematician instrumental in Stone's revolution and the co-inventor of category theory, Saunder Mac Lane, writes "The unity of mathematics is real and depends on wonderful new connections which arise all around us. I urge my friends in logic to look around." (1992, 122).

This gives us two lengthy periods in the history of mathematics resulting in sufficient change, albeit slow and quiet change, to warrant the use of the term *revolution*. While in both cases there is sufficient retention that it would be easy enough for mathematicians at the end of the period to recognise the reasoning of their forebears, say, Hilbert to read Gauss's papers, or Mac Lane to recognise Hilbert's, there is certainly something of the flavour of Kuhn's "transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before". On the other hand, we seem to have departed from it's being "relatively sudden and unstructured."

Considering the two abutting periods, 1800-1900 and 1900-1960, as intervals of long revolutions would seem to entail that mathematics be seen as in a state of permanent revolution. On the other hand, we might consider differences in the relative weight of activity that is either novel exploration or consolidation at certain moments within a period, expecting more exploratory work towards the beginning of one. The mathematician Pierre Cartier tells of his career from the end of one period to the beginning of the next:

> When I began in mathematics the main task of a mathematician was to bring order and make a synthesis of existing material, to create what Thomas Kuhn called *normal science*. Mathematics, in the forties and fifties, was undergoing what Kuhn calls a solidification period. In a given science there are times when you have to take all the existing material and create a unified terminology, unified standards, and train people in a unified style. The purpose of mathematics, in the fifties and sixties, was that, to create a new era of normal science. Now we are again at the beginning of a new revolution. Mathematics is undergoing major changes. We don't know exactly where it will go. It is not yet time to make a synthesis of all these things – maybe in twenty or thirty years it will be time for a new Bourbaki. I consider myself very fortunate to have had two lives, a life of normal science and a life of scientific revolution. (Senechal 1998, 28)

Cartier dates the change in his working style to 1986 (ibid.), so this presolidification period he is estimating to last at least three decades and possibly four. Even to cast only a part of a period as revolutionary is still to see it as a lengthy affair.

Someone who develops Kuhnian thinking to allow for relatively drawn out and structured transformation is Michael Friedman. When in Friedman (2008) he casts the difference between Thomas Kuhn and Ernst Cassirer as one of substantialist against

[14]For a discussion of the nature of such dualities, see Corfield (2017).

functionalist, he is looking to provide a way to mediate. Whereas Cassirer argues for continuity via a sequence of structurally more adequate natural scientific theories, Kuhn argues for radical changes in what is understood to be the basic stuff of the universe. As for mathematics, one might say that the effect of Gray's and Stone's revolutions are that mathematics itself as a whole has moved in a very functionalist direction. Structural relations are preserved, while the stuff of mathematics, for instance, whether geometry describes the space about us is overturned. With this move we might expect from here on a Cassirerian continuity – once structural relations have been located, they may be subsumed within larger ones, but how could they disappear?

However, as far as the physical sciences go, Friedman himself points us in a different direction, as we shall consider in greater detail in section 4. Friedman looks for continuous rational narratives of the discontinuous changes we make to the framework in which we organise the constitutive principles of the field. Discontinuity in physics need not be understood as arising from some Kuhnian *Gestalt switch* between incommensurable theories, but rather from the reasonable decision to adopt a new mathematics allowing the formulation of new constitutive physical principles in which new forms of empirical regularity may be expressed.

Now, something similar may be argued for mathematics. If set theory emerges as a *lingua franca* to express the theories resulting from Gray's revolution, traditional ("material") set theory is found to be poorly equipped to present the results of Stone's "quiet revolution", the intimate structural connections. The need there for a new constitutive language was evident, and was met at that time by category theory. A further revolution from 1960 onwards takes us to today's *homotopical mathematics*, requiring the language of higher category theory, and ultimately, if an associated logical formalism is desired, homotopy type theory. We will return to this discussion in the final section where we look deeper into Friedman's modification of Kuhn's philosophy.

In selecting Gray and Stone's accounts I have been considering mathematics at a global scale. A historically-informed philosophy of mathematics will rest on more local studies, say of a specific branch of mathematics in a particular era. So now let us consider what kind of case studies might help us best to understand the nature of modern mathematics.

3 Case studies

A common conception of what mathematicians do is that they provide proofs of propositions. Awards like the Clay Millennium prizes for seven of the then outstanding mathematical conjectures support such a view. Mathematicians are being rewarded for proving or disproving some statements. This being the case, it is understandable that philosophers focus on the establishment and meaning of such truth claims.

A different view, however, takes the work of mathematicians to include concept-formation and theory-construction. Of course, along the way the truth of propositions will be established, but allowing the expression of such propositions and providing new techniques for such proofs are vital to the mathematical enterprise, even when the propositions happen to be expressible in high-school mathematics. It may seem from the outside that interest in Wiles' proof of Fermat's Last Theorem is confined to the establishment of a simple arithmetic truth about sums of powers of integers, but for number theorists much more important are the tools devised to understand semistable elliptic curves and modular Galois representations. Case studies need to reflect this perspective. Gray, along with the other contributors to Gillies (ed.) (1992), and Stone follow this course, but then the question arises as to the scale, extent and duration someone should choose for their case studies.

A number of problems arise when making such choices. The complexity of any specific passage of mathematical thought makes it very difficult to reconstruct as it occurred, as a historian should see fit to do. Mathematicians endless reformulate and rework theory, make connections to other research, and place ideas in a broader setting. Generally, it's much easier to learn about some chosen concept as it emerges out of the darkness from a later reconfigured, cleaner perspective, often a more general one. But then it's unclear where to stop chasing developments. In the case of modern mathematics, it is generally easier to talk to living mathematicians,[15] but then one is driven right up to present-day understandings.

Physics has the advantage of possessing obviously key moments in its history. As a historical philosopher of modern physics you will need to know about the emergence of quantum theory and general relativity, and ideally of the construction of the standard model. With mathematics it appears less clear which are the developments that simply ought to be known about. One might decide, as I was once advised, that any (rationally reconstructed) case of discovery is as good as another, and so one ought consider theoretical developments from simpler times. However, as explained above, this will cut us off from Stone's "intimate structural connections" and from many of Lautman's reoccurring ideas.

But even having opted for the modern era, examples of such connections and such reoccurrences are plentiful, so the question of which to choose arises. There appears to be no possibility of exhaustion in mathematics, as though rather than the attempt to map a fixed terrain, the task is more like a technology looking to invent tools for an ever-expanding range of functions. Are we left with mathematics post-1930s as anything expressible in set theory? This runs against what is important about participation in Stone's "intimate structural connections," something that accords with a clearly professed value in mathematics. Indeed, prizes are won for discovering deep cases of such connections.

Even if we select significant discoveries of this form of connection, the reader of any case study may need some persuasion that they should invest time in understanding it. One way to provide this is by choosing cases with broader significance. Let's now consider two paths forward in the choice of a case study, (i) via goal-oriented studies, organised as a research programme or a local paradigm, and (ii) via conceptual development.

3.1 Research Programmes and Paradigms

In Chapter 8 of (Corfield, 2003), I considered algebraic topology from the perspective of Lakatos's account of research programmes. There's a problem he generates for himself by taking the hard core of the algebraic topology research programme to be the crystallized axioms of an earlier informal stage, in this case what emerged out of the work recorded in *Proofs and Refutations*. I showed there how little this makes sense of the extraordinary innovative work that leads from the seminal work of Poincaré on what he called *Analysis Situs* to the axiomatic formulations of Eilenberg and Steenrod (1952). Axiomatisations are continually overturned through this period.

[15] Back in the 90s I managed to secure the agreement of Sir Michael Atiyah to help me make philosophical sense of the development of the Atiyah–Singer index theorem. Unfortunately this research wasn't funded.

If a hard core is to be found, something to which all algebraic topologists could sign up, it is not a set of axioms, but rather something like the following: algebraic topology looks to resolve important topological questions by assigning algebraic invariants to spaces. For instance, to determine whether two spaces are homeomorphic, assign homology groups to each and compare. If the results differ, then so do the spaces. If not, look for further invariants, such as cohomology rings to distinguish them. It's not really the solution of notable problems that makes for the importance of this programme, but the general prospect it provides. We see in mid-century algebraic topology the rise of category theory, assignments now being called functors. Perhaps the most interesting aspect of the program is the production of very general tools, in particular (i) homology and cohomology theories and (ii) homotopy theory. These are then carried far and wide across vast swathes of mathematics.

As for rivalry, there is little sign of it as regards what might be seen as the most likely challenger, namely, point-set topology. I was once told that at Oxford, Michael Atiyah would encourage anyone interested in pursuing research in the latter to shift to algebraic topology. This is not so much rivalry in terms of outright competition to determine the best way of treating the same thing. It's a question of what was likely to provide richer conceptual possibilities and so produce the deeper results, and then, as now, that was algebraic topology. This is not to say that point-set topology is worthless, far from it.

This example ends by not fitting terribly well with Kuhnian paradigms or Lakatosian research programmes. Certainly one can see aspects of these philosophers' accounts that are relevant, but the overall tenor of widely-held paradigms folding fairly rapidly under pressure from a new paradigm, or of rivalry between progressive and degenerating programmes competing for the same ground seems off. There just seems to be more 'room' for good work in mathematics.

Nor do such bodies of theory retain their identity. The products of algebraic topology may be transported everywhere. Concerning those working in homotopy theory, Clark Barwick can write

> Some of us call ourselves algebraic topologists, but this has the unhelpful effect of making the subject appear to be an area of topology, which I think is profoundly inaccurate...Today, the praxis of homotopy theory interacts with topology no more often than it does with arithmetic geometry and category theory, and the interactions with areas like representation theory are growing rapidly. Homotopy theory is not a branch of topology. (Barwick 2017, 1)[16]

A similar story can be told about the concepts of homology and cohomology now to be found employed everywhere from number theory to ergodic theory. So we have branches spinning off sub-branches which then exceed their bounds and relate to different branches. It's much more of a weaving development, not at all a linear progression. This makes it difficult to stop short of the global treatments of Gray and Stone from the previous section.

On the other hand, it is not hard to locate the Kuhnian phenomenon of scientists replicating the performance of a successful solution to a puzzle, enshrined as paradigmatic, to a related one. This observation was also taken up by Lakatos in terms of the

[16]Barwick adds "I think of homotopy theory as an enrichment of the notion of equality, dedicated to the primacy of structure over properties. Simplistic and abstract though this idea is, it leads rapidly to a whole universe of nontrivial structures." (Barwick 2017, 1) This idea is precisely what homotopy type theory looks to formalise.

heuristic of a research programmes. We find, for example, in Heller (2022) the author makes the historical claims:

> Felix Klein entrusted many of his students with particular problems arising from his "general idea", and promoted his theory on many occasions – the term Hypergalois Program might be appropriate to describe the broader implications. (Heller 2022, 432)

It is Heller's own language choice to use 'Program' here, an explicit allusion to Lakatos. Interestingly he appeals to an "idea" at the core of the program, not axioms as Lakatos had it.

> The central idea just outlined could be interpreted as the "birth" of the Hypergalois Program. (Heller 2022, 435)

The shift from explicit mathematical propositions to a vaguer idea or aim is something I argue for in my (Corfield 2003, 181).

Mathematicians themselves have also employed the term *program*, suggestive of a future-oriented perspective. One excellent choice of case study to explore, but an immensely daunting one and as yet untouched, would be the case of the so-called *Langlands program*. In terms of scale, it is hard to think of anything in mathematics attracting larger attention today than the Langlands program, dating from the late 1960s. This program has the enormously ambitious goal of uniting of several branches of mathematics, including number theory, harmonic analysis, representation theory, and algebraic geometry.

Anyone looking to tell the tale of precursors of the program will be able to include many of the great names of the 19th century: Gauss, Riemann, Abel, Lie, Klein, Hilbert, etc. They will likely reach further back yet since Wiles' proof of Fermat's last theorem arose from considerations of the Program. This bears witness to a great continuity with the past, and yet the program itself would be unthinkable to formulate even as recently as the 1940s, not least for the crucial concept of *functoriality*, arising as it does out of category theory.

We see again the idea of paradigmatic repetition, here expressed in terms of a *blueprint*:

> The Langlands Program has emerged in recent years as a blueprint for a Grand Unified Theory of Mathematics. Conceived initially as a bridge between Number Theory and Automorphic Representations, it has now expanded into such areas as Geometry and Quantum Field Theory, weaving together seemingly unrelated disciplines into a web of tantalizing conjectures. The Langlands correspondence manifests itself in a variety of ways in these diverse areas of mathematics and physics, but the same salient features, such as the appearance of the Langlands dual group, are always present. This points to something deeply mysterious and elusive, and that is what makes this correspondence so fascinating.
>
> One of the prevalent themes in the Langlands Program is the interplay between the local and global pictures... (Frenkel 2007, v)

There are variants of the blueprint of the Langlands correspondence: global, local, geometric, quantum,... The first two and their interrelations bring us back to Lautman on local-global relations, and indeed many lines of development of his case studies intersect with the Program. Note also the connections to physics – it has been proposed that the *geometric* Langlands correspondence is a consequence of the S-duality of 4D supersymmetric gauge theories, arising from string-theoretic considerations (Kapustin and Witten 2007). A philosophical history of the Langlands program could thus contribute to a sorely needed study of the changing relations between mathematics and physics over the past half a century.

3.2 Concepts

More briefly now, while such research programmes or local paradigms make for very interesting topics for case studies, another path to follow is to focus on the development of a general concept. For instance, in my (2003, Chap. 9) I take on a proposed advance in the study of symmetry which involves the shift from *groups* to *groupoids*. There I detail the kinds of reasons given for this shift, as well as some of the objections made. The topic of symmetry is so central to mathematics that far more can be said. For instance, tensor categories provide a unified framework to discuss notions of quantum symmetry. Then there are higher groups and higher groupoids to consider for higher symmetries.

Another choice of deeply foundational concept is that of *space*. There's a huge amount of mathematical work over the past few decades which develops mathematical conceptions of space. We see exposition of many of these lines of research in the recent *New spaces in mathematics* (Anel and Catren (eds.) 2021a). This volume has a companion, *New spaces in physics* (Anel and Catren (eds.) 2021b), with which naturally there is a great deal of overlap since a good part of the motivation for elaborations of mathematical space over recent decades arises from work done by mathematical physicists. There is a great deal to understand concerning recent conceptualisations of space and symmetry with regard to the changing relationship between mathematics and physics.

In the introduction to their volume on mathematics, the editors point to category theory as the source of all deep changes in geometry over the decades since the mid-twentieth century, when what they call the "classical paradigm" prevailed, so that "If there has been a paradigm shift, it has been the enhancement of *set theory* in *category theory* (in which we include higher categories)", noting there that in all chapters of the book "category theory is central" (Anel and Catren (eds.) 2021a, 10).

> In the classical paradigm, sets can be thought of as the most primitive notion of space – collecting things together in a minimalist way – from which other notions of space are formally derived. In the new paradigm, categories, and in particular higher categories, are the new primitive spatial notion from which the others are derived. Nowadays, categories are everywhere in topology and geometry, from the definition of the basic objects to the problems and methods of study. ((Anel and Catren (eds.) 2021a, 10)

While it is not totally clear from the context whether the authors are self-consciously deploying Kuhnian language here, it seems highly likely. It is true that they use the term 'evolution' rather than 'revolution' in a number of places, such as when asking concerning changes from the "classical paradigm":

> This raises the question of whether the many evolutions undergone by the notion of space since then qualify as a new paradigm. ((Anel and Catren (eds.) 2021a, 10)

However, both together in the introduction and Anel alone in his later chapter speak of 'revolutions'. Indeed the latter alludes to a "deep revolution" in the uptake of higher category theory from which we might "redefine the whole of mathematical structures with an underlying homotopy type instead of an underlying set" (Anel 2021, 548).

About this new *homotopical* way of doing mathematics an exponent can write

> ...homotopical mathematics (whose foundations could be said to be the homotopy theory of type developed by Voevodsky and al.)... reflects a **shift of paradigm** in which the relation of equality relation is weakened to that of homotopy.[1]
>
> [1] It is very similar to the **shift of paradigm** that has appeared with the introduction of category theory, for which 'being equal' has been replaced by 'being naturally isomorphic'. (Toën 2014, 3, my emphasis)

Again, Kuhnian terminology has been deemed appropriate.

I have presented these options of the choice of a unifying quest, such as the Langlands Program, or of a fundamental concept, such as space, as providing opportunities to convey something of what modern mathematics is like: its goals, its sense of progress, its values, its reality. Of course, one will find considerable overlap between these two courses: concepts need to be elaborated to fulfil programmes; programmes generate new versions of old concepts.

But with these choices have we left behind any interest in the foundational topics so dear to traditional philosophers of mathematics? Not at all, as we have seen through the previous sections. Emerging from smaller-scale change, there is an overall thrust in the global development of mathematics from set theory to category theory to higher category theory, and with the latter we have a new claimant for the role of foundational language. In my 2003 book, I end the final chapter on higher category theory by calling for a historically-informed philosophy of mathematics, but also suggest that all of philosophy might gain by looking to exploit the new logic then emerging from this field. I mentioned there the language of *FOLDS* devised by Makkai as one that treats structural equivalence natively. Since that time there has emerged what many consider as a better-adapted such language, *homotopy type theory*, a formalism with structural equivalence baked in.

So it needn't be the case that the selection of case studies is just a sampling from an array of possibilities, like picking out some pretty pebbles from the beach. Given the profound interconnectedness of mathematics, after a time one can start to depict grander narratives. The personal choices outlined above allowed me to be in a position to write on the new language of homotopy type theory: algebraic topology as a research programme; groupoids as an elaboration of the concept of symmetry; category theory and then higher category theory as a organising framework.

With this new logic in hand we now have the opportunity to provide a supporting case study for a philosopher we already introduced above, namely, Michael Friedman.

4 A Post-Kuhnian Interpretation

As we saw towards the end of section 2, Friedman in *Dynamics of Reason* and later writings is looking to provide a post-Kuhnian philosophy of science which gives a central place to a form of philosophical speculation that provides the rationality he sees as missing in Kuhn's account:

> What I call the dynamics of reason is an approach to the history and philosophy of science developed in response to Thomas Kuhn's theory of scientific revolutions. Unlike many philosophical responses to Kuhn, however, my approach, like Kuhn's, is essentially historical. Yet Kuhn's historiography, from my point of view, is much too narrow. Whereas Kuhn focusses primarily on the development of the modern physical sciences from the Copernican revolution to Einsteinian relativity theory, I construct an historical narrative depicting the interplay between the development of the modern exact sciences from Newton to Einstein, on the one side, and the parallel development of modern scientific philosophy from Kant through logical empiricism, on the other. I use this narrative to support a neo-Kantian philosophical conception of the nature of the sciences in question–which, in particular, aims to give an account of the distinctive intersubjective rationality these sciences can justly claim. By contrast, Kuhn's picture led to philosophical challenges to this claim, I argue, precisely because he left out the parallel history of scientific philosophy. (Friedman 2011, 431)

As regards the Einsteinian revolution, he agrees with Kuhn that while it is possible to reconstruct Newtonian physics as an empirical possibility in Einstein's system, allowing us to reject it eventually through observation, this reconstruction is a radical reworking. This reworking allows for a *retrospective* rationality, but it gives no clue as to how the new framework emerged out of the old.

If this were all, we would have incomplete means to justify the transformation. We might satisfy the already converted by reassuring them of a functionalist-structuralist continuity, but those unconverted could continue to hold to the old paradigm from across the divide of the radical incommensurability existing between different systems of reasoning, their standards, modes of explanation and ways of coordinating theory to the world. Hence Friedman demands a *prospective* rationality too, one which explains how it is perfectly possible to think one's way from the old system to the new by correction of the faults of the former. And this is no *post hoc* construction. It is an account of the 'metascientific' work, or 'scientific philosophy', that was instrumental in bringing about the change.

So in a post-Kantian world, we find

- **Metascience**: The likes of Maxwell, Helmholtz, Hertz, Mach and Poincaré rethink the foundations of science, coinciding with changes to geometry due to Riemann, Lie, Klein, Hilbert, etc.
- **Revolution**: Einstein formulates the special and general theories of relativity, resolving conceptual tensions in the field.
- **Philosophy**: Schlick, Carnap and Reichenbach look to make philosophical sense of the revolution.

Contra Quine where we are taken to hold to a network of beliefs, with Friedman we have relations of priority between propositions and so a *historicized a priori*, where mathematics allows the expression of physical constitutive principles with which it is possible to express empirical regularities. What occurs through a change is not of the form of the falsification involved in the *Duhem-Quine thesis*, such as when

- A & B & C implies D

and D is found to be false, so we know that at least one of A, B, C is false, but not which. Rather for Friedman, B will typically be such that it can't even be expressed without A, nor C without A and B.[17]

So rather than theories being seen as long conjunctions, we have an ordering in terms of mathematical language, coordinating principles and empirical laws and regularities. A revolution sees changes in the components that make up its ordered parts, where for instance

- **Mathematical language**: Infinite 3-dimensional Euclidean space + calculus
- **Coordinating principles**: Newton's Laws of motion
- **Empirical laws and regularities**: Law of Gravitation, inertial mass = gravitational mass

is replaced by another framework

- **Mathematical language**: Pseudo-Riemannian manifolds + tensor calculus
- **Coordinating principles**: Invariance of speed of light, Einstein's equivalence principle, freefall as geodesic motion.
- **Empirical laws and regularities**: Field equations, approximately flat time slices.

During a revolution, propositions may change their status:

- **Promotion**: A contingent fact of the Newtonian universe, that the inertial mass and the gravitational mass are the same, becomes a constitutive principle in the Einsteinian picture.
- **Demotion**: On the other hand, the constitutive lack of curvature of the Newtonian universe becomes an approximately true, but in places false, description of this universe.

This for Friedman marks what was right about Kuhn's focus on discontinuity,

> By embedding the old constitutive framework within a new expanded space of possibilities it has, at the same time, entirely lost its constitutive (possibly defining) role. (Friedman 2001, 99)

But the construction of the new framework is brought about rationally by metascientific considerations. And these in turn are grounded in the philosophical reflection that made sense of the previous revolution. Helmholtz, Poincaré and others are living in a post-Kantian world. What's at stake in this process is the vitality not only of science, but also of philosophy. Good things happen to philosophy via this engagement. Without Newton, Kant isn't Kant; without Einstein, Logical Positivism is not the same.

Not only do we have a prospective rationality through change that makes sense in terms of the thought of its day, but there is an overarching rationality due to the whole process, something that eludes Kuhn:

> I want to make clear how the neo-Kantian conception in question presents us with a fundamentally historicized version of scientific intersubjective rationality, so that the standards of objectivity in question are always local and contextual. Nevertheless, in spite of, and even because of, this

[17]For some comments on the idea that we understand this dependence in terms of dependent type theory, see (Corfield 2020, 58-59).

> necessary historicization, the way in which such standards change over time still preserves the trans-historical rationality of the entire process. (Friedman 2011, 432)

So Friedman has found a rational path between Cassirerian functionalist/ structuralist convergence and Kuhnian substantialist discontinuity. However when it comes to mathematics itself, Friedman in *Dynamics of Reason* adopts the purely structuralist approach.

> In pure mathematics, however, there is a very clear sense in which an earlier conceptual framework (such as classical Euclidean geometry) is always translatable into a later one (such as the Riemannian theory of manifolds). In the case of coordinating principles in mathematical physics, however, the situation is quite different. To move to a new set of coordinating principles in a new constitutive framework (given by the principle of equivalence, for example): what counted as coordinating principles in the old framework now hold only (and approximately) as empirical laws, and the old constitutive framework, for precisely this reason, cannot be recovered as such. By embedding the old constitutive framework within a new expanded space of possibilities it has, at the same time, entirely lost its constitutive (possibly defining) role. (Friedman 2001, 99)

Now this, I argue, is a mistake. A similar form of revision of constitutive principles allowing expression of regularities is at play in mathematics.

Cohomology is an excellent case study here. As mentioned in section 3, it emerges out of algebraic topology in the 1930s. At first it is expressed in set-theoretic terms as a certain kind of assignment of a natural number indexed collection of groups, then by 1952 it is organised by category theory as a kind of functor on a suitable category of spaces, and the definition given by axioms written in that language. Without entering into the details, which would require considerable exposition, a couple of generations later and cohomology is now seen as something intrinsic to higher toposes,[18] and so its values expressible in the language of homotopy type theory. Similarly, the much more recent construction known as *differential* cohomology (Hopkins and Singer 2005) is found to be intrinsic to *cohesive* higher toposes,[19] for which a corresponding formalism is *modal* homotopy type theory.

We see the same baking in of principles into the constitutive language with these modifications. As general covariance is baked into general relativity, enforcing the principle of equivalence, here we have the formulation via category theory baking in invariance under isomorphism, and later higher category theory baking in invariance under equivalence. And there are promotions and demotions through the changes. In the first category-theoretic step what was a mere observed regularity is upgraded to appear in a definition as an axiom:

> Eilenberg and Steenrod initiated a new approach by focusing not on the machinery used for the construction of homology or cohomology groups, but on the properties shared by the various theories. The selected a small number of these properties and took them as *axioms* for a theory

[18] "One of the most important geometric achievements of the postclassical period is the revisitation of *algebraic topology* (homotopy and homology theory) in terms of higher category theory" (Anel and Catren (eds.) 2021a, 3). Cohomology amounts to connected components of hom-spaces in $(\infty, 1)$-categories.

[19] See Schreiber (2013).

> of homology and cohomology; they showed that many other properties, formerly separately proved for each theory, were in fact consequences of the axioms, and they examined each theory accordingly to see if it satisfied the axioms. (Dieudonné 2009, 11)

Then what was once taken to be a key feature may be downgraded to a property that only sometimes holds. What was once a *bona fide* homology theory may no longer be seen as such, and so require modification, such as when Čech homology which fails the exactness axiom was modified to strong homology. The further shift to higher-category theory allows a much greater systematisation and expansion of what counts as cohomology (nLab 2022).[20]

But cohomology and differential cohomology aren't merely mathematical concepts. They lie at the heart of modern physics.[21] In particular, *differential* cohomology captures the kind of geometric aspects of a space beyond the merely topological that encode the dynamics of a system:

> Vector bundles and principal bundles have characteristic classes in cohomology; vector bundles with connection and principal bundles with connection have characteristic classes in differential cohomology. (Amabel, Debray & Haine (eds.) 2021, 7)

Differential cohomology may also be used to represent charge quantization (Amabel, Debray & Haine (eds.) 2021, 168-172).

When the physicist Edward Witten asks for a new geometry for physics –

> String theory at its finest is, or should be, a new branch of geometry... I would consider trying to elucidate this proper generalization of geometry as the central problem of physics, certainly the central problem of string theory. (Witten 1988, 95-96)

– from where we are today, at the very least it seems that what is required for this new geometry is *homotopical mathematics* (and also supergeometry) as a setting for the necessary differential cohomology theory, revolutionary mathematics for a revolution in physics. Given the Friedmannian nature of mathematical change that I have proposed, it should be possible then to give the whole package a Friedmannian characterisation.

In later writings, Friedman correctly plays down the difference between mathematics and physics. Concerning his earlier account, he points to two "deeply problematic" aspects, namely, that

> ...it assumes an overly simplified "formalistic" account of modern abstract mathematics, and, even worse, it portrays such abstract mathematics as being directly attached to intuitive perceptible experience at one fell swoop [...]
>
> Our problem, therefore, is not to characterize a purely abstract mapping between an uninterpreted formalism and sensory perceptions, but to

[20] This change does not merely expand possibilities – it also rules out certain options: "In the literature there is a *naive* definition of Lie group cohomology and topological group cohomology, which is not interpretable in terms of hom-spaces in any natural $(\infty, 1)$-category. But later it was found by Segal and then independently by Brylinski that there is a refinement of this definition, which is better behaved. This refinement, it turns out, does have an interpretation in terms of homs in an $(\infty, 1)$-topos." (nLab 2022)

[21] "Cohomology plays a fundamental role in modern physics." (Zeidler 2006, 14). See also (Freed 2000) and (Fiorenza et al. 2020).

> understand the concrete historical process by which mathematical structures, physical theories of space, time, and motion, and mechanical constitutive principles organically evolve together so as to issue, successively, in increasingly sophisticated mathematical representations of experience. (2010, 697-698)

Naturally, one of the major tasks here is to understand what is happening in the quantum transformation of the past century.

As regards quantum physics, Kuhn had used the example of Planck's 1900 theory of black body radiation and its revision six years later, to argue that we might have mathematical continuity accompanying physical discontinuity. He says of the revision "Mathematically it was virtually unchanged... But physically, the entities to which the derivation refers are very different" (Kuhn 2000, 27). This appears then to be a case where Kuhn is playing down Cassirerian structural continuity in favour of the discontinuity of physical interpretation. We might expect Friedman then to provide his own middle path of a metascientifically reasonable shift in the constitutive principles of physics in the post-classical era. However, quantum physics features fairly briefly in *Dynamics of Reason*.[22] Where it does appear it is to remark on a notable difference with relativity theory:

> We have not seen the kind of fruitful interaction between scientific and philosophical or meta-scientific ideas in the case of quantum mechanics that we have seen in the case of the other great revolutions of modern mathematical physics. In the case of the quantum mechanical revolution, in particular, we have not seen *timely* interventions from the philosophical or meta-scientific realm, suitable for rationally bridging the gap between pre-revolutionary and post-revolutionary landscapes. (Friedman 2001, 120)

As such, quantum theory has "had no similar impact on the ongoing practice of philosophy as a discipline" (ibid., 121) to the impact relativity theory has had, for instance, via the response to its success by the Logical Positivists.

Now, an interesting observation made in the editors' introduction of *New Spaces in Physics* (Anel and Catren (eds.) 2021b) is to the effect that the recent study of symplectic geometry is rendering less radical the shift from classical to quantum physics,

> It could even be argued that symplectic geometry opened the path to the comprehension of quantum mechanics as a continuous extension of classical mechanics and no longer as a sort of "new paradigm" discontinuously separated from the classical one. (Anel and Catren (eds.) 2021b, 3-4)

Indeed, now it's classical mechanics that stands in need of reinterpretion (ibid., 5).

Anel and Catren tie this work of recasting the classical-quantum relationship in physics as much closer with what they take to be revolutionary in pure mathematics, as we saw above, the change from set-theoretic to category-theoretic geometry. Even the *classical* notion of the localization of a particle to a point may be retained in quantum physics so long as one follows the *mathematically* revolutionary step of taking 'point' not set-theoretically but rather category-theoretically, where now a 'quantum point' corresponds to a Lagrangian submanifold (Anel and Catren (eds.) 2021b, 4-5). It seems that the revolutionary work is taking place in the transformation of

[22]See in particular pp. 120-123 of Friedman 2001.

mathematics, and this proceeds further to the homotopical mathematics suitable for physics in the form of *higher symplectic geometry* (Calaque 2021, Schreiber 2021).

Although for him no good philosophical use was in fact made of quantum physics, Friedman suggests (2001, 122-123) that von Neumann's proposal in the 1930s of quantum logic as catering for situations where Boolean substructures "cannot be realized or embedded simultaneously within a single comprehensive Boolean structure" was at least of the right *kind* for a timely meta-scientific intervention:

> It engages with other plausibly relevant work in the foundations of the sciences (in this case plausibly relevant work in the contemporaneous foundations of mathematics, and, in more-or-less deliberate interaction with some of the best current philosophical reflection on these matters, it indicates a way in which the idea of a relativized and dynamic a priori can even extend to fundamental principles of logic. (Friedman 2001, 123)

I propose that we are seeing a much stronger candidate of just this kind emerge right now, and one that fits beautifully with his own *Dynamics of Reason* schema: a new logic for a new conception of space for a new physics. The candidate is modal homotopy type theory, which is designed to express higher category-theoretic geometry, where it is possible to formulate the differential cohomology[23] of superspace, a form of which provides a candidate quantum gauge field theory for the role of M-theory (Fiorenza et al. 2019). This latter theory goes by the name of *Hypothesis H*.[24] Quantization then requires the further *linear* homotopy type theory, itself descending from Girard's linear logic.

We are very far from the stage at which the account above – a new logic for a new geometry for a new physics – has achieved recognition. Some components are better-warranted that others, such as the central role of cohomology and then differential cohomology in physics. Of course, any proposed unification of the five superstring theories stands in need of empirical comfirmation,[25] and we have seen raging debates as to whether superstring theory has achieved anything at all yet. As for the logic, almost everything achieved with higher toposes (Lurie 2009) is carried out in a standard *analytic* set-theoretic fashion, rather than the *synthetic homotopy theoretic* way allowed by HoTT, although a degree of success has been achieved here (Corfield 2020, 160-161).

But that such a chain from logic to physics is even a working possibility is an extraordinary finding. To reiterate, it would constitute the kind of account that if it can be made to work would provide an excellent exemplification of Friedman's system in *Dynamics of Reason*, with the modification that sees mathematics play a more vital role. So we now find ourselves faced with an exciting prospect, one requiring a vast amount of work. We would need to fill in all the historical details of the new exemplar, and the metascientific considerations that brought each part into being. To give the briefest sketch of what needs to be covered:

1. Physics: the rise of supersymmetric string theories as a solution to a theory of quantum gravity, and how they are expressed in *differential cohomology*. A proposal for M-theory in this language (Fiorenza et al. 2019).

[23] In full, twisted equivariant differential non-abelian generalized cohomology.

[24] For further resources on this theory, see Schreiber's webpage *Hypothesis H*, and more broadly for the larger vision, see his *differential cohomology in a cohesive topos*.

[25] But for some indications of how Hypothesis H may provide a grounding for topological quantum computing, see Schreiber's webpage Topological Quantum Programming in TED-K.

2. Mathematics: *differential cohomology* and how it may be treated in terms of certain *higher toposes*. How the new homotopic mathematics and supergeometry emerged as parts of broader currents in the transformation of geometry.
3. Logic: *higher toposes* as reasoned about in the language of (modal) *homotopy type theory*. How category-theoretic logic and type theory emerged and then merged.
4. Philosophy: how the language of (modal) *homotopy type theory* makes sense of many currents of thought in metaphysics, philosophy of language, etc. Dependent type theory as emerging from reflection on logic as concerning judgment (e.g., Martin-Löf on Frege and Husserl).
5. Metaphilosophy: how to situate Friedman within the story of philosophy, as a neo-Kantian advance on Kuhn.

In the physics section, for instance, we would need to cover what the editors in their introduction to (Catren and Anel (eds.) 2021b) describe as attempts to quantize general relativity and to geometrize quantum physics, in particular as involving constructions from what they call the post-classical paradigm of mathematics, where category theory prevails. In the logic section, as well as the work of researchers in logic and category theory, we would have to say much about the role of computer science in the rise of type theory.[26] A great amount of very useful existing work is there to be gathered, but the act of framing it all within a single schema is a radical step worth pursuing.

Rather than waiting to start work on writing this up only if and when the triumph of the whole new framework has been acknowledged, in the act of contributing to a Friedmannian account of this prospective framework we can also make contributions on the pre-revolutionary metascientific side. In particular, contributions to what I have written as the philosophical component in (4) are as yet rather thin. As mentioned right at the start of this essay, my attempts in (Corfield, 2020) mark only the very beginning of what is possible. And the philosophical heritage of the dependent type theory stretches right back to Husserl and even as far back as Brentano. Even if only a considerably modified form of this framework could be made to work at some future time, useful work can happen right away.

5 Conclusion

I posed the question at the start of this essay of what a Kuhnian philosophy can bring to modern mathematics. That philosophers should be delving into the history of modern mathematics, I hope to have provided some convincing evidence. That this attention to practice can usefully be extended to very recent times surely then seems reasonable too. We are trying to convey the changing nature of mathematics, and the reasons for those changes, through all ages.

As to whether Kuhn's writings should be our guide in this endeavour, of the later Kuhn's gloss on *revolution* as meaning "relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before" (Kuhn 2000, 17), we may happily take the larger part of this description to apply to episodes in the history of mathematics. What I have argued, for, however is that we allow for slower-paced and better structured transformations.

[26]The inclusion of considerations from technology is in accordance with the extensions to the Dynamics of Reason laid out by Friedman in his (2011).

In so doing I am taking a post-Kuhnian view of matters, along the lines of the account provided by Michael Friedman. Not only do we find changes within mathematics itself as possessing many features of his *Dynamics of Reason* (Friedman 2001), but a particular ongoing transformation to the use of higher category theory and its associated logic, presents a potential new examplar of his account as concerns the physical sciences. The prospect of a new physics being expressed in a new geometry formulated in a new logic has the potential to be beneficial in a great many ways, not least for the discipline of philosophy itself.

Let me end by noting the vital role played in this exemplar by radical transformations in mathematics, and in particular, the successive shifts of foundational framework from set theory to category theory to higher category theory. Reflecting on the methodology of theoretical physics, Einstein remarked

> Experience can of course guide us in our choice of serviceable mathematical concepts; it cannot possibly be the source from which they are derived; experience of course remains the sole criterion of the serviceability of a mathematical construction for physics, but the truly creative principle resides in mathematics. (Einstein 1934, 167)

Perhaps the answer to Friedman's puzzle as to why quantum physics wasn't integrated into philosophy properly is simply that neither the mathematics nor logic were ready. At last they are, presenting us with opportunities for philosophical interpretation, not least to take forward our depiction of the dynamics of reason beyond Friedman's development of Kuhn.[27]

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